The early morning tide marked about 2 inches higher. During the past springs we have

Tuesday, Oct. 10, p.m.	11″ below ...	6″ below ...	5″ ...	E.
Wednesday ,, 11, ,,	5 ,, ...	6 above ...	11 ...	S.S.E.
Thursday ,, 12, ,,	1 ,, ...	12 ,, ...	13 ...	W.N.W.
Friday ,, 13, ,,	3 above ...	9 ,, ...	6 ...	N.N.W.

The comparatively quiet autumnal weather sufficiently accounts for the slight variations.

The tide ebbed as low as 23 feet 6 inches below Trinity in October last year at the London Docks Shadwell entrance, yielding a total tidal vertical oscillation of fully 28 feet in the Port of London.

J. B. REDMAN

6, Queen Anne's Gate, Westminster, S.W., October 19

P.S.—The springs succeeding those described in my letter show a greater difference, influenced doubtless by the great gale of Tuesday, October 24, when the barometer fell as low as in the gales of October 28 and November 16, 1880, on these three occasions reading a tenth under 29 inches. The tide of October 28, 1880, was a low neap, but on November 19, 1880, at the top of the springs estimated at 6 inches under Trinity high water it was 2 feet 9 inches above, or 3 feet 3 inches excess three days after the gale.

The excessive amount of land water now meeting the tide adds to the increase, together with the northerly gales.

	Estimated.	Observed.	Excess.	
Tues. Oct. 24, noon	0′ 9″ below ...	0′ 6″ below ...	0′ 3″	S.S.W. gale.
Wed. ,, 25, p.m.	0 5 above ...	0 12 above ...	0 7	W.S.W.
Thurs. ,, 26, ,,	1 1 ,, ...	2 9 ,, ...	1 8	S.
Fri. ,, 27, ,,	1 6 ,, ...	4 3 ,, ...	2 9	E.N.E.[1]
Sat. ,, 28, ,,	1 7 ,, ...	5 0 ,, ...	3 5	N.N.E.[1]

In effect the last tide is identical with that of January 18, 1881.—J. B. R.

Note.—The estimate of excess due to wind over and above the forecasts is somewhat overstated in this letter, as the Admiralty heights are for London Bridge and those observed are for Westminster, where the reading will be quite 2 inches higher.

[1] Gales.

Umdhlebi Tree of Zululand

THE following note has been communicated to us by the Rev. Dr. Parker, a well-known missionary in Madagascar. The story reminds one of the old myth about the Upas in Java. No light can be thrown upon it at Kew, but perhaps in the pages of NATURE it might meet the eye of some person who could give some more information about it.

W. T. THISELTON DYER

There are two species, in both the leaf is lanceolate, dark green, glossy, hard, and brittle, and from both a thick milky juice exudes, while the fruit is like a long black pod, red at the end. One species is a tree with large leaves, and peculiar looking stem, the bark hanging down in large flakes, showing a fresh growth of bark underneath: in the words of my informant, "What a villainous-looking tree! nasty, rough, ugly!" The other species is a shrub, with smaller leaves, and the bark not peeling off the stem. Both species are said to possess the power of poisoning any living creature which approaches it; the symptoms of poisoning by it being severe headache, blood-shot eyes, and delirium, ending in death. The person affected dies either in delirium, or instantaneously without any delirium. A superstition is connected with this plant. Only a few persons in Zululand are supposed to be able to collect the fruits of the Umdhlebi, and these dare not approach the tree except from the windward side. They also sacrifice a goat or a sheep to the demon of the tree, tying the animal to, or near the tree. The fruit is collected for the purpose of being used as the antidote to the poisonous effects of the tree from which they fall—for only the fallen fruit may be collected. As regards habitat, these trees grow on all kinds of soil, not specially on that which exudes carbonic acid gas, but the tree-like species prefers barren and rocky ground. In consequence of this superstition, the country around one of these trees is always uninhabited, although often fertile.

G. W. PARKER

The Origin of our Vernal Flora

IT is usual to assign an Arctic origin to our mountain flora, and floral comparisons and statistics fully bear out this brilliant generalisation. It is formulated that height above the sea-level is climatally equivalent to northern latitude. This is an assumption that flowering plants are largely conditioned by heat. Thus latitude and oreographical habitats are more or less equal.

Might I introduce another element into this question? Seeing that temperature is so largely influential in explaining the distribution of flowering plants, it occurs to me that not only may height above the sea-level answer to northern distribution, but *seasonal occurrence* as well.

All botanists must have been struck by the fact that the earliest plants to bloom among our vernal flora are genera peculiarly Arctic and Alpine. In some instances (as with *Chrysosplenium oppositifolium* and *C. alternifolium*) the species are identical. These latter plants blossom with us in March or April; within the Arctic circle not until June and July, and even so late as August. Thus, with them, seasonal blossoming is equivalent to northern latitude, as regards the thermal conditions under which they flower. The generic names of all our early flowering plants are those pre-eminently Alpine and Arctic in their distribution—*Potentilla, Stellaria, Saxifraga, Chrysosplenium, Draba, Ranunculus, Cardamine, Alsine,* &c. I contend, therefore, that our vernal flora is explained by the fact that their seasonal occurrence, as regards temperature, is equivalent both to height above the sea-level and northern latitude. In every instance it will be found that the blossoming of the species of the above genera necessarily takes place in Great Britain two or three months earlier than within the polar circle. May we not therefore contend that we owe our English vernal flora to the same causes as distributed our English Alpine plants; and that they are as much protected by being able to flower earlier in the year, as if they had been located on the tops of high hills and mountains?

The power to endure cold and wet displayed by many members of our vernal flora is very remarkable. Thus *Ranunculus bulbosus* and *R. acris, Stellaria media,* &c., are frequently found in flower all through the winter, unless the season be extra cold. Many other early bloomers among our common flowers are also remarkable for their durability, whilst the late flowering plants are equally noticeable for the short space during which they bloom. This indicates a hardihood on the part of our vernal flora which cannot be explained except by reference to the climatal experience of the species. Some of them, as the groundsel and chickweed, may have exchanged an entomophilous for an anemophilous habit, or have become self-fertilised by the change.

Again, it must have been observed that many of our early flowering plants display a tendency towards a seasonal division of labour. All of them either flower before they leaf, or show a tendency to do so, as with the Coltsfoot (*Tussilago farfara*), the Crocus (*C. vernus*), the Snow-drop (*Galanthus nivalis*), &c. Even the violets (*Viola odorata* and *V. canina*), the Daffodil, Primrose, Cowslip, &c., although they in part leaf when they flower, develop leaves much more abundantly after flowering than before, thus showing an inclination towards dividing the period of active life into two distinct stages—the reproductive and the vegetative. Everyone knows how completely this has been effected by the Meadow Saffron (*Colchicum autumnale*). My impression is that this early flowering tendency is a survival of the habit these plants had to blossom under more rigorous climatal conditions. In short, that our vernal flora must have the same origin assigned to it as an Alpine; that it has survived through being able to bloom at an early period of the year at low levels, instead of flowering at a later season higher up, above the sea-level; protection and advantage being secured in both instances.

J. E. TAYLOR

Ipswich

On Coral-eating Habits of Holothurians

BEING struck with a remark of Mr. Darwin in his work on "Coral Reefs," where it is stated on the authority of Dr. J. Allan, of Forres, that the Holothuriæ subsist on living coral, and that by these and other creatures which swarm on coral reefs, an immense amount of coral must be yearly consumed and ground down into mud (p. 14), I determined to commence a series of observations on this subject, in order to ascertain the rate at which these animals void the coral sand from their intestinal canal, and "*ergo*" the amount of coral an individual would yearly transform into sand.

I have by no means satisfied myself that the Holothuriæ do subsist on living coral. This may be due, however, to my field of observation being confined to the fringing reefs around Santa Anna, and the neighbouring coast of the large island of St. Christoval—where living coral occurs only in scanty patches, the greater portion of the coral "flats" being formed of coral detritu

cemented into a more compact rock. I carefully watched the habits of the two species most numerous on the "flats," and in no case did I observe a single individual browsing on the patches of living coral. In truth it was on the dead coral rock forming the "flats" of these reefs that these two species of Holothuriæ subsisted; and it appeared to me that they selected those feeding-grounds where the attachment of molluscs, zoophytes, and stony algæ had to some degree loosened the surface of the rock.

The particular species, on which my observations were made to determine the amount of coral sand daily discharged, possessed a bluish-black body, from 12 to 15 inches in length when undisturbed, and with a circle of 20 pelate tentacles around the mouth. Without going into all the details of my methods of investigation, it will be sufficient to state that from three independent observations on this species of Holothuria I have placed the amount of coral sand daily voided by each individual at not less than two-fifths of a pound (avoirdupois). At this rate some fifteen or sixteen of these animals would discharge a ton of sand from their intestinal canals in the course of a year, which represents about 18 cubic feet of the coral rock forming the "flat" on which these creatures live. In order to illustrate this point more clearly, I will assume that every rood of the surface of the "flat" supports some fifteen or sixteen Holothuriæ, a number which errs rather on the side of deficiency than of excess. In the course of a year 18 cubic feet of coral rock will be removed in the form of sand from the surface of each rood, which is equal to the removal of 1·605th of a foot per annum, or 1 foot in about 600 years.

Although this estimate can be only regarded as of a tentative character and as applicable to but one species of the Holothuriæ, it nevertheless throws some light on what I may term the "organic denudation" of coral reefs, and it is not unreasonable to suppose that where a fringing reef is undergoing a very gradual up-heaval, the combined operation of the fish, the mollusc, the annelid, and the echinoderm, may prevent it from ever attaining an elevation above the level of the sea at high water.

H. B. GUPPY

H.M.S. *Lark*, St. Christoval, Solomon Islands, June 30

Railway Geology—a Hint

IT must often have occurred to others as well as to myself when making a long journey by rail, and being whirled along all too fast through section after section of the greatest interest to the eye that can see in them something more than mere railway "cuttings," how valuable would be some handbook giving the geological features of the country traversed by the principal railway lines, and illustrated by clearly drawn maps and sections.

To give an instance—I have occasion pretty often to travel by the South Western line from Waterloo Station to Exeter, a route along which my untrained eye can take note of a succession of instructive pictures, in the course of a five hours' journey—the recent gravels, &c., covered by pine wood in the neighbourhood of Woking, broken abruptly at Basingstoke station by a section of the chalk, to be succeeded from here onwards to Salisbury by undulating downs of the same formation, bare of trees, and but sparsely inhabited; next, at the Yeovil junction, a sandstone quarry, riddled by martin's nests, presumably of oolitic age; then, between Axminster and Honiton the greyish blue of a cutting through the lias; to be finally succeeded, as I approach the term of my journey, by the rich red earths and loams of the new red sandstone.

Any other line, for instance, the Great Western, which runs parallel to that just instanced, would give equally varied pictures; and a copiously illustrated handbook, with notes explanatory, but as brief as possible—not only of the ground immediately bordering the line of rail, but of the general features of the neighbouring country within the range of the eye of the traveller, should surely, I venture to think, have a large circulation.

Will no geologist—a member of the Government Survey, for instance—undertake the task?

J. C. G.

New University Club, October 27

[We noticed a Guide of this kind for American railways in vol. xix. p. 287, and then suggested the utility of a similar handbook for England.—ED.]

Complementary Colours

I HAVE often noticed the complementary purple on the foam of the bluish-green waters of Alpine rivers. The waters of the Lake of Geneva, and of the Rhone at Geneva, as is well known, are not bluish-green, but greenish-blue; but there also I have noticed what to my eye is exactly the same tint of purple on the foam.

JOSEPH JOHN MURPHY

Old Forge, Dunmurry, co. Antrim, October 28

Palæolithic River Gravels

THE recent articles and reports in your columns on the subject of Palæolithic river gravels bring three points strongly forward, viz.:—

1. The great number of "flint implements" and "flint flakes" found in the river gravels.

2. The presence in the same deposits of bones of recent and extinct Mammalia.

3. The entire absence of the bones of man.

Such being the uniform results of persevering researches extending now for more than twenty-four years, it is surely time to request anthropologists to give (1) some explanation of the remarkable absence of human remains in deposits containing so many objects considered to be of human manufacture, and (2) some proof that it is absolutely impossible for these so-called "flint implements" and "flint flakes" to have been formed by natural causes.

C. EVANS

Hampstead, October 18

LAVOISIER, PRIESTLEY, AND THE DISCOVERY OF OXYGEN

IT is a matter of very little importance whether Lavoisier actually obtained oxygen gas a few weeks or days before Priestley. The bare bald discovery of the gas is a very minor matter when placed in juxtaposition with the astounding revolution produced in chemistry by Lavoisier; with the admirable series of experiments, the acute reasoning, the elegant logical penetration, which enabled him to overthrow the theory of Phlogiston when literally all Europe supported it. The discovery of oxygen dims and pales before the development of the theory of combustion, the theories of acidification, of calcination, of respiration, and the introduction of exact quantitative processes and instruments of precision into chemistry.

But it matters much whether the fair fame of one of the noblest and wisest men in the long roll of illustrious natural philosophers is to remain with a grievous slur cast upon it. It matters much whether his reputation is to be blasted by the reproach that he claimed the discovery of oxygen, knowing well that Priestley had preceded him.

It is with a view of removing this slur upon the memory of the founder of modern chemistry, and certainly not with any thought of adding one iota to his long list of greater triumphs, that we have examined into the true bearings of the question.

First as to the accusations. Dr. Thomas Thomson, in his "History of Chemistry," 2nd edit., 1830, vol. ii. p. 19, writes: "Lavoisier, likewise, laid claim to the discovery of oxygen gas, but his claim is entitled to no attention whatever, as Dr. Priestley informs us that he prepared this gas in M. Lavoisier's house in Paris, and showed him the method of procuring it in the year 1774, which is a considerable time before the date assigned by Lavoisier for his pretended discovery." Again, p. 106: "Yet in the whole of this paper the name of Dr. Priestley never occurs, nor is the least hint given that he had already obtained oxygen gas by heating red oxide of mercury. So far from it, that it is obviously the intention of the author of the paper to induce his readers to infer that he himself was the discoverer of oxygen gas. For after describing the process by which oxygen gas was obtained by him, he says nothing further remained but to determine its nature, and 'I discovered with *much surprise* that it was not capable of combination with water by agitation,' &c. Now why the expression of surprise in describing phenomena which had been already shown? And why the omission of all mention of Dr. Priestley's name? I confess that this seems to me capable of no other explanation